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now abandoned, which he made concerning my earlier paper, says:

This assumption was supported by uncertainty as to meaning and by lack of homogeneity of his expression for the potential function introduced on page 342 of his first paper; and still more by his identification of astronomic with geocentric latitude (on p. 339, same paper) by means of the loose phrase "with sufficient approximation." A similar lack of "accuracy and precision" will be found in several parts of his latest paper cited above. See, for example, his equations (*j*), wherein he confounds geocentric with reduced latitude; also p. 199, where he identifies his equations (38) and (41) with my equation (26) and makes with respect to them the surprising statement, "it is, of course, evident that this function corresponds to some distribution of revolution" in the earth's mass.

I shall reply first to the criticism concerning the "identification of astronomic with geocentric latitude." After having derived (in my first paper) a general formula for the meridional deviation of a falling body, I assigned various particular forms to the potential function and thus obtained the formulæ for the meridional deviations corresponding to these particular potential functions. Some of these potential functions were expressed in terms of astronomic latitude, and others in terms of geocentric. Consequently, the same thing was true of the corresponding formulæ for the meridional deviation. For instance, the formula of Gauss was expressed in terms of astronomic latitude and several others were expressed in terms of geocentric latitude. In order to compare the magnitudes given by the special formulæ I replaced, in the formula of Gauss, the symbol representing astronomic latitude by that representing geocentric, and in so doing I used the expression "with sufficient approximation" for which I am now criticized. It is of course evident that by this procedure a slight error was made in the formula of Gauss *after* its rigorous form had been derived. But none of the other work was thereby affected, the derivation of the general formula as well as that of each of the special formulæ being strictly rigorous. Concerning the criticism about my equations (*j*) I wish to

say that the parameter  $\psi$  may be regarded as a geocentric latitude, since it is measured at the center of the spheroid and from the equatorial plane. I did not say that it was the geocentric latitude of the point ( $\tau$ ,  $\sigma$ ). However, it would have been well to mention that it is called the reduced latitude of the point ( $\tau$ ,  $\sigma$ ). But even if the reader interprets it as the geocentric latitude of the point ( $\tau$ ,  $\sigma$ ), the argument in which it is used will not thereby be vitiated. For, as I pointed out, the relation (*l*) in which it is used is approximate, the relation (*n*) being the exact relation approximated. Now, the error made in using relation (*l*) instead of relation (*n*) is twice as great as the error made in relation (*l*) by calling  $\psi$  the geocentric instead of the reduced latitude of the point ( $\tau$ ,  $\sigma$ ). As regards the "surprising statement," I should like to point out that on page 192<sup>14</sup> I defined a distribution of revolution as one for which  $\partial V / \partial \lambda \equiv 0$ , and surely my function (38) satisfies this condition since it does not contain the longitude  $\lambda$ . Then I was very particular to say—in the last foot-note on page 199—that for the assumption  $B = A$  made by Dr. Woodward in his relations (31), his potential function (26) is the same as my potential function (38). Concerning the potential function introduced on page 342 of my first paper, I stated that it had been taken from Poincaré, "Figures d'Equilibre d'une Masse Fluide" (1902), Chapt. V. Following Poincaré, I used the symbol *M* where Dr. Woodward used the symbol *M<sub>K</sub>*. In other words, I suppressed the gravitation constant. But it was easy to see from the expressions and values of the constants that no error had been made in so doing.

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#### VEGETATIVE REGENERATION OF ALFALFA

WHEN growing alfalfa plants in the greenhouse, for infection experiments with the crown-gall of alfalfa (*Urophlyctis alfalfæ*), the writer found it desirable to clip the shoots at intervals in order to secure a multiplication of the adventitious buds from the crown.

<sup>14</sup> *Astronomical Journal*, Nos. 670-72.

Some of the portions of the leaves and stems clipped from the plants dropped upon the soil of the pots and were allowed to remain there. Some days later it was found that a number of these fragments had put out roots from the cut surfaces and were developing into healthy shoots. As the plant under consideration is of great economic importance, a further experiment was tried in order to see if the regeneration of shoots from cut fragments of alfalfa is easily induced. A handful of fragments cut from an entirely different group of alfalfa plants was scattered loosely over the surface of a pot of well-watered soil. The fragments were watered from day to day, care being taken to avoid altering their positions. After a week it was found that fragments of several descriptions had rooted firmly and were developing into healthy shoots. The regenerating fragments included portions of stems, portions of petioles, petioles with blades attached, leaflets without petioles attached, and even small portions of the leaf blade.

So far as the writer has been able to learn, multiplication by this vegetative method has not been reported of alfalfa previous to this time. Under favorable conditions, such regeneration might assume considerable importance in the field. Especially suggestive is the possibility of strengthening a stand in an irrigated district by an early cutting followed by free watering. The cut portions might be left where they fall, or collected and scattered over areas where the stand is thin.

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#### QUOTATIONS

##### THE ORGANIZATION OF SCIENCE IN GREAT BRITAIN

At last, on all sides, it is being recognized that we should organize our scientific resources. Had the suggestion which I made to this effect on January 20 been acted on—that is to say, if the Royal Society had grouped the whole body of its fellows (mainly according to subjects) in grand committees and set these to work—we should have been many months in advance of our present position, and not a little might have been done to apply science to

the numerous problems which are only now being hinted at in public. In view of our scientific conservatism, it is impossible to blame the uninstructed masses and difficult to find much fault even with our rulers.

It is well known that the men who are versed in the chemistry and properties of explosive materials and who might, during all these months, have rendered the greatest service in perfecting their production and in improving them have not been consulted. One of the two men to whom the service ammunition of all the armies of to-day is due is still with us and an active scientific worker; in no other country would it have been possible that such a man should not have been called into consultation. Many of us might have been of assistance if only as foremen in works—technical foremen have been badly needed.

It is imperative that the strongest body of technical opinion that we can bring together should be behind the War Office and the authority that is charged with the supply of explosives. It is to be hoped that Mr. Lloyd George is now alive to this need and of the deadly peril in which we stand if it be neglected. The suggestion has been made that Lord Haldane should be invited to preside over a committee of concentrated scientific intellect to deal with war problems. But Lord Haldane, as a lawyer, would be entirely out of place as chairman of such a body; it must be in expert hands to be of service. The government is not competent to select the members of such a commission. I believe the Royal Society to be the only competent advisory body under the circumstances—it is our scientific House of Peers, and if it can not either itself furnish sufficient competent men or provide them from the junior ranks of science, the sooner it is declared defunct the better.

I would again urge that the society be organized forthwith as a whole; not only is this the only way of eliminating personal differences, but it is the only way of getting at the ideas latent in our scientific community. No half-dozen or so persons, at the present time, have the right to assume that they can do all that is required in any branch of science; no